solid state of selenium which evolves heat on crystallizing—all appear to be homologues, at once, of liquid water below 32°, and of the glassy state of matter.

Should this hypothesis be verified, water below 32°, or rather, perhaps, from the temperature of maximum density downwards through that of freezing, may have to be regarded as the type of the vitreous condition of matter; and the causes of the peculiar characters of that condition, its effects on the transmission of the vibrations of sound and light, the conchoidal fracture, &c., may have to be discovered by researches on its molecular nature.

III. "On the Effect of the presence of Metals and Metalloids upon the Electric Conductivity of Pure Copper." By A. Matthiessen, Esq., and M. Holzmann, Esq. Communicated by Professor Wheatstone. Received March 14, 1860.

(Abstract.)

After studying the effect of suboxide of copper, phosphorus, arsenic, sulphur, carbon, tin, zinc, iron, lead, silver, gold, &c., on the conducting power of pure copper, we have come to the conclusion *that there is no alloy of copper which conducts electricity better than the pure metal.*

May 3, 1860.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

In accordance with the Statutes, the Secretary read the names of the Candidates recommended by the Council for Election into the Society, viz.—

Frederick Augustus Abel, Esq.
Thomas Baring, Esq., M.P.
John Frederic Bateman, Esq.
Edward Brown-Séquard, M.D.
Richard C. Carrington, Esq.
Francis Galton, Esq.
Joseph Henry Gilbert, Esq.
Sir William Jardine, Bart.
Thomas Hewitt Key, Esq.
Joseph Lister, Esq.
Rev. Robert Main, M.A.
Robert William Mylne, Esq.
Roundell Palmer, Esq., Q.C.
John Thomas Quekett, Esq.
Edward Smith, M.D.

The following communications were read :—

I. "On the relations between the Elastic Force of Aqueous Vapour, at ordinary temperatures, and its Motive Force in producing Currents of Air in Vertical Tubes." By W. D. CHOWNE, M.D., F.R.C.P. Communicated by JOHN BISHOP, Esq. Received March 15, 1860.

(Abstract.)

In 1853 the author of this communication made a considerable number of experiments which demonstrated that when a tube, open at both ends, was placed vertically in the undisturbed atmosphere of a closed room, there was an upward movement of the air within the tube of sufficient force to keep an anemometer of light weight in a state of constant revolution, though with a variable velocity. An abstract of the results of these experiments was printed in the 'Proceedings' of the Society for June 1855.

In order to further investigate the immediate cause or nature of the force which set the machine in motion, the author instituted a series of fresh experiments.

These experiments were made in the room described in the former communication, guarded in the same manner against disturbing causes, and with such extra precautions as will be hereafter explained. The apparatus used was a tube 96 inches long and 6·75 inches uniform diameter, the material zinc. The upper extremity was open to its full extent; at the lower, the aperture was a lateral one only, into which a piece of zinc tube 3 inches in diameter, and bent once at right angles, was accurately fitted with the outer orifice upward. Within this orifice, which was about 5 inches above the level of the floor, an anemometer, described in the former paper, and weighing 7 grains, was placed in the horizontal position. About midway between the upper and the lower extremity of the tube, a very delicate differential thermometer was firmly and permanently fixed, with one bulb outside and the other inside, and the aperture through which the latter was inserted completely closed. The scale was on the stem of the outer bulb.

The results of a long series of observations were recorded. The state of the dry and the wet bulb of the hygrometer, as well as the indications of the differential thermometer, was noted, in connexion with the number of revolutions performed per minute by the ane-

mometer. While the differential thermometer indicated the same relative differences between the heat of the atmosphere within and without the tube, the velocity of the revolutions was found to vary considerably. This variation was discovered to be chiefly, if not wholly, dependent on the *elasticity* of vapour, due to the hygrometrical state of the atmosphere, as estimated from the dry- and the wet-bulb thermometers, and calculated from the tables of Regnault.

240 observations were recorded and afterwards separated into groups, each group comprising those in which the differential thermometer gave the same indication.

If in either of these groups we separate into two classes the cases in which the elasticity was highest, from the cases in which it was lowest, and multiply the mean of each with the corresponding mean of the number of the revolutions of the anemometer, their product is nearly a constant, thus showing that the velocity of ascent of the atmospheric vapour is inversely as its elasticity; and hence it follows that the velocity of the ascending current in the tube varies inversely as the density or elastic force of the vapour suspended in the atmosphere. This was rendered evident by the aid of Tables appended to the paper.

When the mean elastic force of vapour calculated from the dry and the wet bulbs is multiplied by the constant, 13·83, the result gives the whole amount of water in a vertical column of the atmosphere in inches; it follows therefore that when the difference of temperature between the external air and that in the tube, as shown by the differential thermometer, is constant, the velocity of the current in the tube varies inversely as the weight of the vapour suspended in the atmosphere.

In an Appendix the author describes some additional experiments, made with the view of ascertaining whether the readings of the differential thermometer were mainly due to actual changes of temperature within the tube, or to extraneous causes acting on the external bulb. He found that when the external bulb was covered with woollen cloth or protected by a zinc tube of about 4 inches diameter and 6 inches long, the temperature of the bulb was increased about 2° on the scale of the instrument, and that when they were removed the prior reading was restored, while the number of revolutions of the anemometer per minute was not appreciably

affected by the change. This explains why the readings of the differential thermometer varied from 33°·0 to 33°·5 as described in the paper, without producing a corresponding change in the velocity of the anemometer.

For the purpose of obtaining a more correct estimate of the influence of a given increase of heat within the tube, the author introduced into the tube at its lowest extremity, a phial containing eight ounces of water at the temperature of 100° Fahr., corked so that no vapour could escape. The result showed that in thirteen observations a quantity of heat equal to an increase of one-tenth of a degree on the scale of the differential thermometer, was equivalent to a mean velocity of the anemometer of 3·6 revolutions per minute, the greatest number being 3·8, the least 3·3 per minute.

These observations render it still more evident, that if a higher temperature within the tube had been the main cause of the revolutions of the anemometer, the variations in their velocity would not have been in such exact relation to the elastic force of the atmospheric vapour, as has been shown to be the case. They also lead to the inference, that the apparent excess of heat within the tube alluded to by the author in his Paper read before the Society in 1855 did not really exist, and to the conclusion that, if such excess had been present, the anemometer would not have been brought to a state of rest by depriving the air of the room of a portion of the moisture ordinarily suspended in it.

II. "On the Relation between Boiling-point and Composition in Organic Compounds." By Hermann Kopp, Esq. Communicated by Dr. Hofmann. Received March 20, 1860.

(Abstract.)

The author was the first to observe (in 1841) that, on comparing pairs of analogous organic compounds, the same difference in boiling-point corresponds frequently to the same difference in composition. This relation between boiling-point and composition, when first pointed out, was repeatedly denied, but is now generally admitted. The continued experiments of the author, as well as of numerous other inquirers, have since fixed many boiling-points which had hitherto